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XXIII. *Electro-Chemical Researches, on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia.*  
By Humphry Davy, Esq. Sec. R. S. M. R. I. A.

Read June 30th, 1808.

1. *Introduction.*

IN the Philosophical Transactions for 1807, Part I. and 1808, Part I. I have detailed the general methods of decomposition by electricity, and stated various new facts obtained in consequence of the application of them.

The results of the experiments on potash and soda, as I stated in my last communication to the Society, afforded me the strongest hopes of being able to effect the decomposition both of the alkaline and common earths; and the phenomena obtained in the first imperfect trials made upon those bodies countenanced the ideas that had obtained from the earliest periods of chemistry, of their being metallic in their nature.\*

\* BECCHER is the first chemist, as far as my reading informs me, who distinctly pointed out the relations of metals to earthy substances, see Phys. subt. Lipsiæ, 4to. p. 61. He was followed by STAHL, who has given the doctrine a more perfect form. BECCHER's idea was that of an universal elementary earth, which, by uniting to an inflammable earth, produced all the metals, and under other modifications formed stones. STAHL admitted distinct earths which he supposed might be converted into metals by combining with phlogiston; see STAHL Fundament. Chym. p. 9. 4to. and Conspect. Chem. i. 77. 4to.—NEUMAN gives an account of an elaborate series of unsuccessful experiments which he made to obtain a metal from quicklime. LEWIS

Many difficulties however occurred in the way of obtaining complete evidence on this subject: and the pursuit of the enquiry has required much labour and a considerable devotion of time, and has demanded more refined and complicated processes than those which had succeeded with the fixed alkalies.

The earths like the fixed alkalies are non-conductors of electricity; but the fixed alkalies become conducting by fusion: the infusible nature of the earths, however, rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxygene, made it unavailing to act upon them in solution in water; and the only methods that proved successful, were those of operating upon them by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity, in metallic alloys, so as to obtain evidences of their nature and properties.

NEUMAN's Chem. Works, 2d. edit. vol. i. p. 15. The earlier English chemical philosophers seem to have adopted the opinion of the possibility of the production of metals from common earthy substances; see BOYLE, vol. i. 4to. p. 564, and GREW, Anatomy of Plants, lec. ii. p. 242. But these notions were founded upon a kind of alchemical hypothesis of a general power in nature of transmuting one species of matter into another. Towards the end of the last century the doctrine was advanced in a more philosophical form; BERGMAN suspected barytes to be a metallic calx, Præf. Sciagraph. Reg. Min. & Opusc. iv. 212. BARON supported the idea of the probability of alumine being a metallic substance, see Annales de Chimie, vol. x. p. 257.--LAVOISIER extended these notions, by supposing the other earths metallic oxides. Elements, 2d edit. KERR's translation, p. 217. The general enquiry was closed by the assertion of TONDI and RUPRECHT, that the earths might be reduced by charcoal; and the accurate researches of KLAPROTH and SAVARESI, who proved by the most decisive experiments, that the metals taken for the bases of the earths were phosphurets of iron, obtained from the bone ashes and other materials employed in the experiment, Annales de Chimie, vol. viii p. 18. and vol. x. p. 257, 275. Amidst all these hypotheses, potash and soda were never considered as metallic in their nature; LAVOISIER supposed them to contain azote; nor at that time were there any analogies to lead that acute philosopher to a happier conjecture.

I delayed for some time laying an account of many of the principal results which I obtained before the Society, in the hopes of being able to render them more distinct and satisfactory; but finding that for this end a more powerful battery, and more perfect apparatus than I have a prospect of seeing very soon constructed, will be required, I have ventured to bring forwards the investigation in its present imperfect state; and I shall prefer the imputation of having published unfinished labours, to that of having concealed any new facts from the scientific world, which may tend to assist the progress of chemical knowledge.

*2. Methods employed for decomposing the alkaline Earths.*

Barytes, strontites, and lime, slightly moistened, were electrified by iron wires under naphtha, by the same methods, and with the same powers as those employed for the decomposition \* of the fixed alkalies. In these cases, gas was copiously evolved, which was inflammable; and the earths where in contact with the negative metallic wires became dark coloured, and exhibited small points having a metallic lustre, which, when exposed to air, gradually became white; they became white likewise when plunged under water, and when examined in this experiment by a magnifier, a greenish powder seemed to separate from them, and small globules of gas were disengaged.

In these cases there was great reason to believe that the earths had been decomposed; and that their bases had combined with the iron, so as to form alloys decomposable by the oxygene of air or water; but the indistinctness of the effect,

\* See page 4.

and the complicated circumstances required for it, were such as to compel me to form other plans of operation.

The strong attraction of potassium for oxygene, induced me to try whether this body might not detach the oxygene from the earths, in the same manner as charcoal decomposes the common metallic oxides.

I heated potassium in contact with dry pure lime, barytes, strontites, and magnesia, in tubes of plate glass; but as I was obliged to use very small quantities, and as I could not raise the heat to ignition without fusing the glass, I obtained in this way no good results. The potassium appeared to act upon the earths and on the glass, and dark brown substances were obtained, which evolved gas from water; but no distinct metallic globules could be procured: from these circumstances, and other like circumstances, it seemed probable, that though potassium may partially de-oxygenate the earths, yet its affinity for oxygene, at least at the temperature which I employed, is not sufficient to effect their decomposition.

I made mixtures of dry potash in excess and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit in the same manner as that I employed for obtaining the metals of the alkalies. My hopes were, that the potassium, and the metals of the earths might be de-oxygenated at the same time, and enter into combination in alloy.

In this way of operating, the results were more distinct than in the last: metallic substances appeared, less fusible than potassium, which burnt the instant after they had formed, and which by burning produced a mixture of potash and the earth employed; I endeavoured to form them under naphtha, but

without much success. To produce the result at all, required a charge by the action of nitric acid, which the state of the batteries did not permit me often to employ;\* and the metal was generated only in very minute films, which could not be detached by fusion, and which were instantly destroyed by exposure to air.

I had found in my researches upon potassium, that when a mixture of potash and the oxides of mercury, tin, or lead, was electrified in the *VOLTAIC* circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium was obtained; the attraction between the common metals and the potassium apparently accelerating the separation of the oxygene.

The idea that a similar kind of action might assist the decomposition of the alkaline earths, induced me to electrify mixtures of these bodies and the oxide of tin, of iron, of lead, of silver, and of mercury; and these operations were far more satisfactory than any of the others.

A mixture of two-thirds of barytes and one-third of oxide

\* The power of this combination, though it consisted of one hundred plates of copper and zinc of six inches, and one hundred and fifty of four inches, at this time was not more than equal to that of a newly constructed apparatus of one hundred and fifty, of four inches. It had been made for the demonstrations in the Theatre of the Royal Institution in 1803; and since that time had been constantly employed in the annual courses of Lectures, and had served in different parts, for the numerous experiments on the decomposition of bodies by electricity, detailed in the *BAKERIAN* Lectures for 1806 and 1807, and a number of the plates were destroyed by corrosion. I mention these circumstances, because many chemists have been deterred from pursuing experiments on the decomposition of the alkalies and the earths, under the idea that a very powerful combination was required for the effect. This, however, is far from being the case; all the experiments detailed in the text may be repeated by means of a *VOLTAIC* battery, containing from one hundred to one hundred and fifty plates of four or six inches.

of silver very slightly moistened was electrified by iron wires ; an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point. When the iron wire to which this substance adhered was plunged into water containing a little alum in solution, gas was disengaged, which proved to be hydrogene ; and white clouds which were found to be sulphate of barytes, descended from the point of the wire.

A mixture of barytes and red oxide of mercury, in the same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance, that produced barytes by exposure to air, with the absorption of oxygene ; and which occasioned the evolution of hydrogene from water, leaving pure mercury, and producing a solution of barytes.

Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air or water, with like phenomena ; but the quantities of metallic substances obtained were exceedingly minute ; they appeared as mere superficial formations surrounding the point of the wire, nor did they increase after the first few minutes of electrization, even when the process was carried on for some hours.

These experiments were made previous to April, 1808, at which time the batteries were so much injured by constant use, as no longer to form an efficient combination. The enquiry was suspended for a short time : but in May I was enabled to resume it, by employing a new and much more powerful combination,

constructed in the Laboratory of the Royal Institution, and consisting of five hundred pairs of double plates of six inches square.

When I attempted to obtain amalgams with this apparatus, the transmitting wires being of platina, of about  $\frac{1}{40}$  of an inch in diameter; the heat generated was so great as to burn both the mercury and basis of the amalgam at the moment of its formation; and when by extending the surfaces of the conductors, this power of ignition was modified, yet still the amalgam was only produced in thin films, and I could not obtain globules sufficiently large to submit to distillation. When the transmitting wires were of iron of the same thickness, the iron acquired the temperature of ignition, and combined with the bases of the earths in preference to the mercury, and metallic alloys of a dark grey colour were obtained, which acted on water with the evolution of hydrogen, and were converted into oxide of iron, and alkaline earths.

Whilst I was engaged in these experiments, in the beginning of June, I received a letter from Professor BERZELIUS of Stockholm, in which he informed me that in conjunction with Dr. PONTIN, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them, and that in this way he had obtained amalgams of the metals of these earths.

I immediately repeated these operations with perfect success; a globule of mercury, electrified by the power of the battery of 500, weakly charged, was made to act upon a surface of slightly moistened barytes, fixed upon a plate of platina. The mercury gradually became less fluid, and after a few minutes was found covered with a white film of barytes;



and when the amalgam was thrown into water, hydrogen was disengaged, the mercury remained free, and a solution of barytes was formed

The result with lime, as these gentlemen had stated, was precisely analogous.

That the same happy methods must succeed with strontites and magnesia, it was not easy to doubt, and I quickly tried the experiment.

From strontites I obtained a very rapid result; but from magnesia, in the first trials, no amalgam could be procured. By continuing the process however, for a longer time, and keeping the earth continually moist, at last a combination of the basis with mercury was obtained, which slowly produced magnesia by absorption of oxygen from air, or by the action of water.

All these amalgams I found might be preserved for a considerable period under naphtha. In a length of time, however, they became covered with a white crust under this fluid. When exposed to air, a very few minutes only were required for the oxygenation of the bases of the earths. In water the amalgam of barytes was most rapidly decomposed: that of strontites and that of lime next in order: but the amalgam from magnesia, as might be expected from the weak affinity of the earth for water, very slowly changed; when a little sulphuric acid was added to the water, however, the evolution of hydrogen, and the production and solution of magnesia were exceedingly rapid, and the mercury soon remained free.

I was inclined to believe that one reason why magnesia was less easy to metallize than the other alkaline earths, was its insolubility in water, which would prevent it from being presented in the nascent state, detached from its solution at the

negative surface. On this idea I tried the experiment, using moistened sulphate of magnesia, instead of the pure earth; and I found that the amalgam was much sooner obtained. Here the magnesia was attracted from the sulphuric acid, and probably deoxygenated and combined with the quicksilver at the same instant.

The amalgams of the other bases of the alkaline earths, could, I found, be obtained in the same manner from their saline compounds.

I tried in this way very successfully, muriate and sulphate of lime, the muriate of strontites, and of barytes, and nitrate of barytes. The earths separated at the deoxygenating surface, there seemed instantly to undergo decomposition, and seized upon by the mercury, were in some measure defended from the action of air, and from the contact of water, and preserved by their strong attraction for this metal.

### *III. Attempts to procure the Metals of the alkaline Earths; and on their Properties.*

To procure quantities of amalgams sufficient for distillation, I combined the methods I had before employed, with those of M. M. BERZELIUS and PONTIN.

The earths were slightly moistened, and mixed with one-third of red oxide of mercury, the mixture was placed on a plate of platina, a cavity was made in the upper part of it to receive a globule of mercury, of from fifty to 60 grains in weight, the whole was covered by a film of naphtha, and the plate was made positive, and the mercury negative, by a proper communication with the battery of five hundred.

The amalgams obtained in this way, were distilled in tubes

of plate glass, or in some cases in tubes of common glass. These tubes were bent in the middle, and the extremities were enlarged, and rendered globular by blowing, so as to serve the purposes of a retort and receiver.

The tube after the amalgam had been introduced, was filled with naphtha, which was afterwards expelled by boiling, through a small orifice in the end corresponding to the receiver, which was hermetically sealed when the tube contained nothing but the vapour of naphtha, and the amalgam.

I found immediately that the mercury rose pure by distillation from the amalgam, and it was very easy to separate a part of it; but to obtain a complete decomposition was very difficult.

For this nearly a red heat was required, and at a red heat the bases of the earths instantly acted upon the glass, and became oxygenated. When the tube was large in proportion to the quantity of amalgam, the vapour of the naphtha furnished oxygene sufficient to destroy part of the bases: and when a small tube was employed, it was difficult to heat the part used as a retort sufficient to drive off the whole of the mercury from the basis, without raising too highly the temperature of the part serving for the receiver, so as to burst the tube.\*

In consequence of these difficulties, in a multitude of trials, I obtained only a very few successful results, and in no case could I be absolutely certain that there was not a minute portion of mercury still in combination with the metals of the earths.

\* When the quantity of the amalgam was about fifty or sixty grains, I found that the tube could not be conveniently less than one-sixth of an inch in diameter, and of the capacity of about half a cubic inch.

In the best result that I obtained from the distillation of the amalgam of barytes, the residuum appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness, in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis, in the first degree of oxygenation.\*

\* From this fact, compared with other facts that have been stated, p. 336, it may be conjectured, that the basis of barytes has a higher affinity for oxygene than sodium; and hence, probably the bases of the earths will be more powerful instruments for detecting oxygene, than the bases of the alkalies.

I have tried a number of experiments on the action of potassium on bodies supposed simple, and on the undecomposed acids. From the affinity of the metal for oxygene, and of the acid for the substance formed, I had entertained the greatest hopes of success. It would be inconsistent with the object of this paper to enter into a full detail of the methods of operation; I hope to be able to state them fully to the Society at a future time, when they shall be elucidated by further researches; I shall now merely mention the general results, to shew that I have not been tardy in employing the means which were in my power, towards effecting these important objects.

When potassium was heated in muriatic acid gas, as dry as it could be obtained by common chemical means, there was a violent chemical action with ignition; and when the potassium was in sufficient quantity, the muriatic acid gas wholly disappeared, and from one-third to one-fourth of its volume of hydrogen was evolved, and muriate of potash was formed.

On fluoric acid gas, which had been in contact with glass, the potassium produced a similar effect; but the quantity of hydrogen generated was only one-sixth or one-seventh of the volume of gas, and a white mass was formed, which principally consisted of fluuate of potash and silice, but which emitted fumes of fluoric acid when exposed to air.

When boracic acid, prepared in the usual manner, that had been ignited, was heated in a gold tube with potassium, a very minute quantity of gas only was liberated, which was hydrogen, mixed with nitrogen, (the last probably from the common air in the tube); borate of potash was formed, and a black substance, which became white by exposure to air.

When exposed to air, it rapidly tarnished, and fell into a white powder, which was barytes. When this process was conducted in a small portion of air, the oxygene was found absorbed, and the nitrogene unaltered; when a portion of it was introduced into water, it acted upon it with great violence and sunk to the bottom, producing in it barytes; and hydrogen was generated. The quantities in which I obtained it were too minute for me to be able to examine correctly, either its physical or chemical properties. It sunk rapidly in water, and even in sulphuric acid, though surrounded by globules of hydrogen, equal to two or three times its volume; from which it seems probable, that it cannot be less than four or five times as heavy as water. It flattened by pressure, but required a considerable force for this effect.

In all these instances there is great reason to believe that the hydrogen was produced from the water adhering to the acids; and the different proportions of it in the different cases, are a strong proof of this opinion. Admitting this idea, it seems that muriatic acid gas must contain at least one-eighth or one-tenth of its weight of water; and that the water oxygenates in the experiment a quantity of potassium, sufficient to absorb the whole of the acid.

In the cases of fluoric and boracic acids, there is probably a decomposition of these bodies; the black substance produced from the boracic acid is similar to that which I had obtained from it by electricity. The quantities that I have operated upon, have been as yet too small to enable me to separate and examine the products, and till this is done, no ultimate conclusion can be drawn.

The action of potassium upon muriatic acid gas, indicates a much larger quantity of water in this substance, than the action of electricity in Dr. HENRY's elaborate experiments; but in the one instance the acid enters into a solid salt, and in the other it remains aeriform; and the difficulty of decomposition by electricity, must increase in proportion as the quantity of water diminishes, so that at the apparent maximum of electrical effect, there is no reason to suppose the gas free from water.

Those persons who have supposed hydrogen to be the basis of muriatic acid may, perhaps, give another solution of the phenomena, and consider the experiment I have detailed as a proof of this opinion.

The metal from strontites sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxydation.

The metal from lime, I have never been able to examine exposed to air or under naphtha. In the case in which I was able to distil the quicksilver from it to the greatest extent, the tube unfortunately broke, whilst warm, and at the moment that the air entered, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quicklime.

The metal from magnesia seemed to act upon the glass, even before the whole of the quicksilver was distilled from it. In an experiment in which I stopped the process before the mercury was entirely driven off, it appeared as a solid, having the same whiteness and lustre as the other metals of the earths. It sunk rapidly in water, though surrounded by globules of gas, producing magnesia, and quickly changed in air, becoming covered with a white crust, and falling into a fine powder, which proved to be magnesia.

In several cases in which amalgams of the metals of the earths, containing only a small quantity of mercury were obtained, I exposed them to air on a delicate balance, and always found that during the conversion of metal into earth, there was a considerable increase of weight.

I endeavoured to ascertain the proportions of oxygene, and bases, in barytes and strontites, by heating amalgams of them in tubes filled with oxygene, but without success. I satisfied myself, however, that when the metals of the earths were burned in a small quantity of air they absorbed oxygene,

gained weight in the process, and were in the highly caustic or unslacked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids.

The evidence for the composition of the alkaline earths is then of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar, the inflammable matters in all cases separating at the negative surface in the VOLTAIC circuit, and the oxygene at the positive surface.

These new substances will demand names; and on the same principles as I have named the bases of the fixed alkalies, potassium and sodium, I shall venture to denominate the metals from the alkaline earths barium, strontium, calcium, and magnium; the last of these words is undoubtedly objectionable, but magnesium \* has been already applied to metallic manganese, and would consequently have been an equivocal term.

#### IV. *Enquiries relative to the Decomposition of Alumine, Silex, Zircon, and Glucine.*

I tried the methods of electrization and combination with quicksilver, and the common metals, by which I had succeeded in decomposing the alkaline earths, on alumine and silix; but without gaining distinct evidences of their having undergone any change in the processes.

Obliged to seek for other means of acting upon them, it was necessary to consider minutely their relations to other

\* BERGMAN. Opusc. tom. ii. p. 200.

bodies, and to search for analogies by which the principles of research might be guided.

Alumine very slowly finds its point of rest at the negative pole, in the electrical circuit ; but silex, even when diffused in its gelatinous state through water, rests indifferently at the negative or positive poles.

From this indifference to positive and negative electrical attractions, following the general order of facts, it might be inferred, that if these bodies be compounds, the electrical energies of their elements are nearly in equilibrium ; and that their state is either analogous to that of insoluble neutral salts, or of oxides nearly saturated with oxygene.

The combinations of silex and alumine, with acids and alkalis, as well as their electrical powers, were not inconsistent with either of these ideas ; for in some respects they resemble in physical characters, fluat and phosphate of lime, as much as in others, they approach to the oxides of zinc and tin.

On the idea that silex might be an insoluble neutrosaline compound, containing an unknown acid or earth, or both, and capable of being resolved into its secondary elements, in the same manner as sulphate of barytes, or fluat of lime, I made the following experiments.

Two gold cones,\* connected by moistened amianthus, were filled with pure water, and placed in the electrical circuit, a small quantity of carefully prepared and well washed silex was introduced into the positive cone : the action was kept up from a battery of two hundred plates, for some hours till nearly half of the fluid in each cone was exhausted ; the remainders were examined ; the fluid in the cone containing

\* The same as those described in Phil. Trans. 1807, p. 6.



the silex was strongly acid ; that in the opposite cone was strongly alkaline ; the two fluids were passed through bibulous paper, and mixed together, when a precipitate fell down, which proved to be silex.

On the first view of the subject, it appeared probable that this silex had been formed by the union of the acid and the alkaline matter in the two cones, and that the experiment demonstrated a decomposition and recomposition of silex ; but before such a conclusion could be made, many points were to be determined.

It was possible that the acid might be nitric acid, produced as in other electrical experiments of a similar nature, and that this acid might have dissolved silex, which was precipitated by the alkaline matter at the other pole, which might be either potash used for dissolving the silex, which had adhered to it, notwithstanding the processes of lixiviation in acids, or ammonia produced in consequence of the presence of the atmosphere ; or if potash was present, it was likewise possible that the silex might have been carried over in solution, with this alkali, from the positive to the negative surface.

Minute experiments were instituted and completed in the same manner as those detailed in the Philosophical Transactions for 1807, p. 7. which soon proved that there was no reason to suppose that the silex had been changed in these experiments.

The acid proved to be nitric acid, which under the electrical action seemed to have dissolved the silex ; the alkali turned out to be principally fixed alkali ; and that it was merely an accidental ingredient, and not a constituent of the silex, appeared from this circumstance, that when the same portion

of silex was long electrified, by degrees it lost its power of affording the substance in question.\*

This result having taken place, the same plan of operation was not pursued with respect to alumine, which resembles a saline compound less than silex, and the method which I now adopted of acting upon these bodies, was on the supposition of their being inflammable substances so highly saturated with oxygene as to possess little or no positive electricity.

Alumine and silex have both a strong affinity for potash and soda; now supposing them to be oxides, it was reasonable to conclude that the oxygene, both in the alkalies and the earths, must be passive as to this power, which must consequently be referred to their bases, and on this notion it was possible that it might be made to assist their decomposition by electricity.

After this reasoning, I fused a mixture of one part of silex, and six of potash in a platina crucible, and preserved the mixture fluid, and in ignition, over a fire of charcoal; the crucible was rendered positive from the battery of five hundred, and a rod of platina, rendered negative, was brought in contact with

\* If silex that has been carefully washed, after precipitation by muriatic acid from liquor silicum, be moistened, and acted on by mercury negatively electrified, the mercury soon contains a notable quantity of potassium. Well washed alumine that has been precipitated from alum by carbonate of soda, affords by the same treatment sodium and potassium, so that the powers of electrochemical analysis are continually demonstrating the imperfection of the common chemical methods of separating bodies from each other. The purest boracic acid which can be obtained from borax by chemical decomposition, by electrical analysis is shewn to contain both soda, and the decomposing acid employed in the process; and hence the experiment on the action of the boracic acid and potassium, page 343, may possibly be explained without assuming its decomposition.

the alkaline menstruum. At the moment of contact there was a most intense light; when the rod was plunged into the liquid an effervescence took place, and globules which burnt with a brilliant flame rose to the surface, and swam upon it in a state of combustion. In a few minutes, when the mixture was cool, the platina bar was removed: after as much as possible of the alkali and silex had been detached from it by a knife, there remained brilliant metallic scales round it, which instantly became covered with a white crust in the air, and some of which inflamed spontaneously. The platina appeared much corroded, and of a darker tint than belongs to the pure metal. When it was plunged into water it strongly effervesced: the fluid that came from it was alkaline; when a few drops of muriatic acid were added to the solution, a white cloudiness occurred, which various trials demonstrated, depended upon the presence of silex.

A similar mixture of potash and alumine was experimented upon in the same manner, and the results were perfectly analogous; there adhered to the rod of platina a film of a metallic substance, which rapidly decomposed water, and afforded a solution which deposited alumine by the action of an acid.

I tried several forms of this experiment, with the hopes of being able to obtain a sufficient quantity of the metallic matter from the platina, so as to examine it in a separate state; but I was not successful. It was always in superficial scales, which oxidated, becoming white and alkaline, before it could be detached in the air; it instantly burnt when heated, and could not be fused under naphtha or oil.

I tried similar experiments with mixtures of soda and

alumine, and soda and zircon, and used iron as the negatively electrified metal. In all these cases, during the whole process of electrization, abundance of globules, which swam in a state of inflammation on the fused mass, were produced. And in the mixture, when cooled, small laminæ of metal were found of the colour of lead, and less fusible than sodium, which adhered to the iron; they acted violently upon water, and produced soda and a white powder, but in quantities too small to be minutely examined.

I endeavoured to procure an alloy of potassium, and the bases of the earths, from mixtures of potash, silex, and alumine, fused by electricity, and acted on by the positive and negative surfaces in the same manner as pure potash, in experiments for the decomposition of that substance; but I obtained no good results. When the earths were in quantities equal to one-fourth or one-fifth of the alkali, they rendered it so highly non-conducting, that it was not easy to affect it by electricity, and when they were in very minute portions, the substance produced had the characters of pure potassium.

I heated small globules of potassium, in contact with silex and alumine, in tubes of plate glass filled with the vapour of naphtha: the potassium seemed to act at the same time upon the glass and the earths, and a grayish opaque mass, not possessed of metallic splendour was obtained, which effervesced in water, depositing white clouds. Here it was possible that the potash had been converted wholly or partly into protoxide, by its action upon the earths; but as no globule was obtained, and as the plate glass alone might have produced the effect, no decided inference of the decomposition of the earths can be drawn from the process.

I shall now mention the last trials that I made with respect to this object.

Potassium, amalgamated with about one-third of mercury, was electrified negatively under naphtha, in contact with silex very slightly moistened, by the power of five hundred ; after an hour the result was examined. The potassium was made to decompose water, and the alkali formed neutralized by acetous acid ; a white matter, having all the appearance of silex precipitated, but in quantity too small for accurate examination.

I tried the same method of action upon alumine and glucine, and obtained a cloudiness, more distinct than in the case of silex, by the action of an acid upon the solution obtained from the amalgam.

Zircone exposed in the same manner to the action of electricity, and the attraction of potassium, furnished still more satisfactory results, for a white and fine powder, soluble in sulphuric acid, and which was precipitated from sulphuric acid by ammonia, separated from the amalgam that had been obtained, by the action of water.

From the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude, that alumine, zircone, glucine, and silex are, like the alkaline earths, metallic oxides, for on no other supposition is it easy to explain the phenomena that have been detailed.

The evidences of decomposition and composition, are not, however of the same strict nature as those that belong to the fixed alkalies and alkaline earths ; for it is possible, that in the experiments in which the silex, alumine, and zircone appeared

to separate during the oxidation of potassium and sodium, their bases might not actually have been in combination with them, but the earths themselves, in union with the metals of the alkalies, or in mere mechanical mixture. And out of an immense number of experiments which I made of the kind last detailed, a very few only gave distinct indications of the production of any earthy matter; and in cases when earthy matter did appear, the quantity was such as rendered it impossible to decide on the species.

Had I been so fortunate as to have obtained more certain evidences on this subject, and to have procured the metallic substances I was in search of, I should have proposed for them the names of silicium, alumium, zirconium, and glucium.

*V. On the production of an Amalgam from Ammonia, and on its Nature and Properties.*

IN the communication from Professor BERZELIUS and Dr. PONTIN, which I have already referred to, a most curious and important experiment on the deoxydation and amalgamation of the compound basis of ammonia is mentioned, which these ingenious gentlemen regard as a strict proof of the idea I had formed of its being an oxide with a binary basis.

Mercury, negatively electrified in the VOLTAIC circuit, is placed in contact with solution of ammonia. Under this agency it gradually increases in volume, and when expanded to four or five times its former dimensions, becomes a soft solid.

And that this substance is composed of the deoxygenated compound basis of ammonia and mercury, they think is proved, 1. By the reproduction of quicksilver and ammonia,

with the absorption of oxygene, when it is exposed to air; and secondly, by its forming ammonia in water, whilst hydrogen is evolved, and the quicksilver gradually becomes free.

An operation, in which hydrogen and nitrogen exhibit metallic properties, or in which a metallic substance is apparently composed from its elements, cannot fail to fix the attention of chemists: and the peculiar interest which it offered in its relations to the general theory of electrochemical science, induced me to examine the circumstances connected with it minutely and extensively.

In repeating the process of the Swedish chemists, I found that to form an amalgam from fifty or sixty grains of mercury, in contact with saturated solution of ammonia, required a considerable time, and that this amalgam greatly changed even in the short period required for removing it from the solution.

I was however able, in this mode of operating, to witness all the results they have stated, and I soon found simple and more easy means of producing the effect, and circumstances under which it could be more distinctly analysed.

The experiments which I have detailed in the BAKERIAN lecture for 1806, proved that ammonia is disengaged from the ammoniacal salts, at the negative surface in the VOLTAIC circuit; and I concluded that under this agency, it may be acted on in what is called the nascent state, when it was reasonable to conclude it would be more readily deoxygenated and combined with quicksilver.

On this view of the subject, I made a cavity in a piece of muriate of ammonia; into this a globule of mercury, weighing about fifty grains, was introduced. The muriate was slightly

moistened, so as to be rendered a conductor, and placed on a plate of platina, which was made positive in the circuit of the large battery. The quicksilver was made negative by means of a platina wire. The action of the quicksilver on the salt was immediate; a strong effervescence with much heat took place. The globule in a few minutes had enlarged to five times its former dimensions, and had the appearance of an amalgam of zinc; and metallic crystallizations shot from it, as a centre, round the body of the salt. They had an arborescent appearance, often became coloured at their points of contact with the muriate; and when the connection was broken, rapidly disappeared, emitting ammoniacal fumes, and reproducing quicksilver.

When a piece of moistened carbonate of ammonia was used, the appearances were the same, and the amalgam was formed with equal rapidity. In this process of deoxydation, when the battery was in powerful action, a black matter formed in the cavity, which there is every reason to believe was carbonaceous matter from the decomposition of the carbonic acid of the carbonate.\*

The strong attraction of potassium, sodium, and the metals of the alkaline earths for oxygen, induced me to examine whether their deoxydating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and the result was very satisfactory.

When mercury, united to a small quantity of potassium, sodium, barium, or calcium, was made to act upon moistened

\* The black matter which separates at the negative surface in the electrical experiments on the decomposition of potash or soda, and which some experimenters have found it difficult to account for, is I find carbonaceous, and dependent upon the presence of carbonic acid in the alkali.



muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

As in these cases, however, a portion of the metal used for the deoxydation always remained in union in the compound; in describing the properties of the amalgam from ammonia, I shall speak only of that procured by electrical means.

The amalgam from ammonia, when formed at the temperature of  $70^{\circ}$  or  $80^{\circ}$ , is a soft solid, of the consistence of butter; at the freezing temperature it becomes firmer, and a crystallized mass, in which small facets appear, but having no perfectly defined form.\* Its specific gravity is below 3, water being one.

When exposed to air it soon becomes covered with a white crust, which proves to be carbonate of ammonia.

When thrown into water it produces a quantity of hydrogen, equal to about half its bulk, and in consequence of this action the water becomes a weak solution of ammonia.

When it is confined in a given portion of air, the air enlarges considerably in volume, and the pure quicksilver re-appears. Ammoniacal gas, equal to one and a half or one and three-fifths of the volume of the amalgam is found to be produced, and a quantity of oxygen equal to one-seventh, or one-eighth of the ammonia disappears.†

\* From the facet I suspect the form to be cubical. The amalgam of potassium crystallizes in cubes as beautiful, and in some cases as large, as those of bismuth.

† This experiment confirms the opinions I have stated concerning the quantity of oxygen in ammonia; but as water is present, as will be immediately shewn, the data for proportions are not perfectly correct.

When thrown into muriatic acid gas, it instantly becomes coated with muriate of ammonia, and a small quantity of hydrogen is disengaged.

In sulphuric acid it becomes coated with sulphate of ammonia and sulphur.

I attempted by a variety of modes to preserve this amalgam. I had hoped by submitting it to distillation out of the contact of air, or water, or bodies which could furnish oxygen, to be able to obtain the deoxygenated substance which had been united to the quicksilver in a pure form; but all the circumstances of the experiment opposed themselves to such a result.

It is well known to persons accustomed to barometrical experiments, that mercury after being once moistened, retains water with great perseverance, and can only be freed from it by boiling; and in the cases of the decomposition of ammonia, when a soft amalgam had been kept continually moist, both internally and externally for some time, it could not be expected that all the water adhering to it should be easily removed.

I wiped the amalgam as carefully as possible with bibulous paper; but even in this process a considerable portion of the ammonia was regenerated; I attempted to free it from moisture by passing it through fine linen, but a complete decomposition was effected, and nothing was obtained but pure quicksilver.

The whole quantity of the basis of ammonia combined in sixty grains of quicksilver, as is evident from the statements that have been made, does not exceed  $\frac{1}{200}$  part of a grain, and to supply oxygen to this scarcely  $\frac{1}{1000}$  part of a grain of water would be required, which is a quantity hardly appre-

ciable, and which merely breathing upon the amalgam would be almost sufficient to communicate.

Hence, when an amalgam, which had been wiped by means of bibulous paper, was introduced into naphtha, it decomposed almost as rapidly as in the air, producing ammonia and hydrogen.

In oils it evolved hydrogen, and generated ammoniacal soap; and when it was introduced into a glass tube, closed by a cork, gas was rapidly formed, and the mercury remained free; and this gas, when examined, was found to consist of from about two-thirds to three-fourths ammonia, and the remainder hydrogen.\*

That more moisture sometimes existed attached to the amalgam, when wiped as dry as possible by bibulous paper, than was sufficient for the effect of decomposition, I soon found by an experiment of distillation.

About a quarter of a cubic inch of an amalgam nearly solid was wiped very dry, and introduced into a small tube: in this tube it was heated till the gaseous matter had expelled the quicksilver; the tube was then closed, and suffered to cool, when moisture, which proved to be a saturated solution of ammonia, had precipitated upon it.

I have mentioned that the amalgams obtained from ammonia, by means of the metals of the fixed alkalies or alkaline earths, seemed to contain much more ammoniacal basis in combination than those procured by electricity: and when they are combined with the metals of the fixed alkalies or

\* In the experiment of the action of the amalgam upon air, the oxygen is probably absorbed by nascent hydrogen, and reproduces water, which is dissolved by the ammonia.

of the earths in any considerable quantities, they are much more permanent.

Triple compounds of this kind, when carefully wiped, scarcely produce any ammonia under naphtha, or oil, and may be preserved for a considerable time in closed glass tubes, a little hydrogen being the only product evolved from them.

I heated a triple amalgam obtained from ammonia by potassium, and which had been wiped by bibulous paper in a dry plate-glass tube over mercury; a considerable elevation of temperature was required before any gaseous matter was emitted, but the heat was raised till gas was rapidly formed, and the whole of the amalgam expelled from the tube: in cooling, the mercury rose very quickly in it, so that a great part of the gaseous matter had been either mercury or water, in vapour, or something which the mercury had absorbed in cooling. The small quantity which was permanent, did not equal one half the volume of the amalgam.

On the idea that this gas might be a compound of hydrogen and nitrogen in the state of deoxygenation, I mixed a small quantity of oxygen gas with it, but no change of volume took place; I then exposed it to naphtha, when one half of it was absorbed, which by the effect the naphtha produced upon turmeric must have been ammonia; the remaining gas analyzed was found to consist of the oxygen that had been introduced, and of hydrogen and nitrogen to each other in the proportion of nearly four to one.

At first I was perplexed by this result, which seemed to prove the production of ammonia, independent of the presence of any substance which could furnish oxygen to it, and to shew that its amalgamation was merely owing to its being

freed from water, and combined with hydrogen: but a satisfactory solution of the difficulty soon offered itself. Exposing the triple amalgam procured from ammonia by potassium to a concentrated solution of ammonia, I found that it had very little action upon it, and introducing the amalgam moistened by it into a glass tube, it had nearly the same permanency as the amalgam which had been wiped before it was introduced, a little hydrogen only being evolved; but on heating the tube gaseous matter was rapidly generated, which proved to consist of two-thirds ammonia, and one-third hydrogen.

In the instance in which the amalgam had been wiped, a small quantity of solution of ammonia, and perhaps of potash, must have adhered to it; and though the amalgam does not act upon this powerfully at common temperatures, yet when the water is raised in vapour, it tends to oxygenate both the basis of ammonia, and potassium, and hence hydrogen is evolved, and volatile alkali produced.

I distilled an amalgam procured by potassium from ammonia, in a tube filled with the vapour of naphtha, and hermetically sealed, in the same manner as in the experiments for obtaining the metals of the earths, but in this case I procured ammonia, hydrogen, and nitrogen only, and pure mercury; and the residuum was potassium, which acted powerfully on the glass tube.

In another experiment of the same kind, I kept one part of the tube cool by ice, at the time the other part was strongly heated, but nothing condensable except mercury was produced, and the elastic products were the same as in the former instance.

I endeavoured to procure an amalgam from ammonia, to

which no moisture could be supposed to adhere, by heating an amalgam of potassium in ammoniacal gas. The amalgam became covered with a film of potash, but it did not enlarge in its dimensions, and a considerable quantity of non-absorbable gas, which was found to consist of five parts of hydrogen, and one of nitrogen, was produced. The amalgam after this operation did not emit ammonia by exposure to air, hence it seems probable, that for the deoxygenation of ammonia, and the combination of its basis with mercury, the alkali must be in the nascent state, or at least in that condensed form in which it exists in ammoniacal salts, or solutions.

VI. *Some Considerations of general Theory, connected with the Metallization of the Alkalies and the Earths.*

THE more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about  $\frac{1}{12000}$  part of its weight of new matter, is rendered a solid, yet has its specific gravity diminished from 13.5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

It is scarcely possible to conceive that a substance which forms with mercury so perfect an amalgam, should not be metallic in its own nature;\* and on this idea to assist the discus-

\* The nature of the compounds of sulphur and phosphorus with mercury, favours this opinion; these inflammable bodies by combination, impair its metallic properties; cinnabar is a non conductor, and it would seem from PELLÉTIER'S experiments, Ann. de Chimie, vol. xiii. p. 125, that the phosphuret of mercury is not metallic in its characters; charcoal is a conductor, and in plumbago carbon approaches very

discussion concerning it, it may be conveniently termed ammonium.

But on what do the metallic properties of ammonium depend?

Are hydrogen and nitrogen both metals in the æriform state, at the usual temperatures of the atmosphere, bodies of the same character, as zinc and quicksilver would be in the heat of ignition?

Or are these gases, in their common form, oxides, which become metallized by deoxygenation?

Or are they simple bodies not metallic in their own nature, but capable of composing a metal in their deoxygenated, and an alkali in their oxygenated state?

These problems, the second of which was stated by Mr. CAVENDISH to me, and the last of which belongs to Mr. BERZELIUS, offer most important objects of investigation.

I have made some experiments in relation to them, but as yet unsuccessfully. I have heated the amalgam of potassium, in contact with both hydrogen and nitrogen, but without attaining their metallization; but this fact cannot be considered as decisively for or against any one of these conjectures.

I mentioned in the BAKERIAN Lecture for 1807, that a modification of a phlogistic chemical theory might be defended on the idea that the metals and inflammable solids, usually called simple, were compounds of the same matter as that existing in

near to a metal in its characters, so that the metallic nature of steel does not militate against the reasoning in the text. The only facts which I am acquainted with, that do militate against it, are the metallic characters of some of the sulphurets and phosphurets of the imperfect metals.

hydrogene, with peculiar unknown bases, and that the oxides, alkalies, and acids were compounds of the same bases with water, and that the phænomena presented by the metals of the fixed alkalies might be explained on this hypothesis.

The same mode of reasoning may be applied to the facts of the metallization of the earths and ammonia, and perhaps with rather stronger evidences in its favour, but still it will be less, distinct and simple, than the usually received theory of oxygenation, which I have applied to them.

The general facts of the combustion, and of the action of these new combustible substances upon water, are certainly most easily explained on the hypothesis of LAVOISIER ; and the only good arguments in favour of a common principle of inflammability, flow from some of the novel analogies in electrochemical science.

Assuming the existence of hydrogen in the amalgam of ammonium, its presence in one metallic compound evidently leads to the suspicion of its combination in others. And in the electrical powers of the different species of matter, there are circumstances which extend the idea to combustible substances in general. Oxygen is the only body which can be supposed to be elementary, attracted by the positive surface in the electrical circuit, and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxygen. Hydrogen is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxygen ; may not then the different inflammable bodies, supposed to be simple, contain this as a common element ?

Should future experiments prove the truth of this hypo-



thesis, still the alkalies, the earths, and the metallic oxides will belong to the same class of bodies. From platina to potassium there is a regular order of gradation as to their physical and chemical properties, and this would probably extend to ammonium, could it be obtained in the fixed form. Platina and gold in specific gravity, degree of oxidability, and other qualities, differ more from arsenic, iron, and tin, than these last do from barium and strontium. The phenomena of combustion of all the oxidable metals are precisely analogous. In the same manner as arsenic forms an acid by burning in air, potassium forms an alkali and calcium an earth; in a manner similar to that in which osmium forms a volatile and acrid substance by the absorption of oxygen, does the amalgam of ammonium produce the volatile alkali; and if we suppose that ammonia is metallized, by being combined with hydrogen, and freed from water, the same reasoning will likewise apply to the other metals, with this difference, that the adherence of their phlogiston or hydrogen, would be exactly in the inverse ratio of their attraction for oxygen. In platina \* it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any

\* The common metallic oxides are lighter than their bases, but potash and soda are heavier; this fact may be explained on either theory; the density of a compound will be proportional to the attraction of its parts. Platina, having a weak affinity for oxygen, cannot be supposed to condense it in the same degree as potassium does; or if platina and potassium be both compounds of hydrogen, the hydrogen must be attracted in platina, with an energy infinitely greater than in potassium. Sulphuric acid is lighter than sulphur; but phosphoric acid (where there is a stronger affinity) is heavier than phosphorus. The oxide of tin (wood tin) is very little inferior to tin in specific gravity. In this instance the metallic base is comparatively light, and the attraction for oxygen strong; and in a case when the metal is much lighter and the attraction for oxygen stronger, it might be expected a priori that the oxide would be heavier than the base.

of the metals without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalies, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

Whatever new lights new discoveries may throw upon this subject, still the facts that have been advanced, shew that a step nearer at least has been attained towards the true knowledge of the nature of the alkalies and the earths.\*

\* Since the facts in this Paper were communicated to the Royal Society, I have seen an account of some very curious experiments of M.M. GAY LUSSAC, and THENARD, (in Number 148 of the *Moniteur*, for 1808, which I have just received,) from one of which they have concluded, "that potassium may be a compound of hydrogen and potash."

These gentlemen are said to have heated potassium in ammonia, and found that the ammonia was absorbed, and that hydrogen gas equal to two-thirds of its volume appeared, and that the potassium by this process had become of a grayish-green colour. By heating this grayish-green substance considerably, two-fifths of the ammonia were again emitted, with a quantity of hydrogen and nitrogen corresponding to one-fifth more, and by adding water to the mixture, and heating it very strongly again, they obtained the remainder of the ammonia, and nothing but potash was left.

In these complex processes, the phenomena may be as easily explained on the idea of potassium being a simple, as that of its being a compound substance; nor when the facts that have been stated in this paper and those about to be stated, are considered, can the view of these distinguished chemists, as detailed in the notice referred to, be at all admitted.

Potash, as I have found by numerous experiments, has no affinity for ammonia, for it does not absorb it when heated in it; it is not therefore (allowing their theory) possible to conceive that a substance having no attraction for potash, should repel from it a substance which is intimately combined with it, and which can be separated in no other way.

A part of the hydrogen evolved in their experiment, may be furnished by water contained in the ammonia; but it is scarcely possible that the whole of it can be derived from this source, for on such an idea the ammonia must contain more than half its weight of water. There is however no evidence that the whole of the hydrogen may

Something has been separated from them which adds to their weight ; and whether it be considered as oxygene, or as

not be furnished by the decomposition of the volatile alkali itself. Potassium in its first degree of oxygenation, may have an affinity for nitrogene, or potassium may expel a portion of hydrogen at the moment of its combination with ammonium ; and as the whole of the ammonia cannot be regenerated without the presence of water ; hydrogen and a little oxygene may be furnished to the remaining elements of the ammonia, from the water, and oxygene to the potassium.

Even before the conclusion was formed, that a metallic substance is decomposed in this experiment, it should have been proved that the nitrogene had not been altered.

That mere potash, combined with hydrogen, cannot form potassium, is I think shewn by an experiment which I tried, in consequence of the important fact lately ascertained by M. M. GAY LUSSAC, and THENARD, of the deoxydation of potash by iron.

An ounce of potash was kept in ignition for some time in an iron tube, ground into a gun barrel in which one ounce and a half of iron turnings were ignited to whiteness ; a communication was opened, by withdrawing a wire which closed the tube containing the potash, between that alkali and the metal.

As the potash came in contact with the iron, gaseous matter was developed, which was received in a proper apparatus, and though some of it was lost by passing through the potash into the atmosphere, yet nearly half a cubic foot was preserved, which proved to be hydrogen. In the tube were found two products, one in the quantity of a few grains, containing potassium, combined with a small quantity of iron, and which had sublimed in the operation, and the other a fixed white metallic substance which consisted of an alloy of iron and potassium.

The first of these substances burnt when thrown upon water ; and in its other characters resembled pure potassium, except that its specific gravity was greater, its colour less brilliant, and when it tarnished in the atmosphere, it became of a much deeper colour than pure potassium.

Now potash that has been ignited, is the purest form known of this alkali ; but on M. M. GAY LUSSAC's and THENARD's theory, this potash must contain water, not only sufficient to furnish hydrogen to metallize the alkali, but likewise the quantity disengaged : dry potash then, as it is procured in our experiments, must on this theory be a compound, containing a considerable quantity of matter which can furnish hydrogen ; and what would be its form or properties if deprived of this matter we are wholly unable to judge, which brings this question to the general question discussed in the text.

water, the inflammable body is less compounded, than the unflammable substance resulting from its combustion.

Potassium I find may be produced readily from dry ignited potash in electrical experiments; and the result of the combustion of potassium in oxygene gas is an alkali, so dry that it produces violent heat, and ebullition when water is added to it.

In M.M. GAY LUSSAC's and THENARD's experiment on the action of potassium on ammonia, the hydrogene disengaged in the first process, and that existing in the ammonia disengaged in the second process, exactly equals the whole quantity contained in the ammonia. But there is no proof of any hydrogene being disengaged from the potassium, for the ammonia lost is not generated, nor potash formed, but by the addition of a substance, consisting of oxygene and hydrogene; and as the three bodies concerned in this experiment are potassium, ammonia, and water, the result ought to be potash, ammonia, and a quantity of hydrogene, equal to that evolved by the mere action of water on potassium, which is said to be the case.

Even if there were no other proofs, the chemical properties of potassium are so wholly unlike those that might be expected from a compound of potash and hydrogene, that they are almost sufficient to decide the question. Potassium acts upon water with much more energy than potash, and produces much more heat in it; and yet if a compound of hydrogene, the affinity of potash for water must be diminished by its affinity for hydrogene, to say nothing of the quantity of heat, which ought (on the common theory of capacity for heat) to be carried off by this light inflammable gas.

Potassium burns in carbonic acid, and precipitates charcoal from it; whereas hydrogene electrized with carbonic acid, converts it into gaseous oxide of carbon.

Potash has a very slight attraction for phosphorus; but potassium has a very strong affinity for it, so as to separate it from hydrogene; and according to M.M. GAY LUSSAC and THENARD, with the phænomena of inflammation. Potash has no affinity for arsenic, yet from the experiments of these gentlemen, it appears that potassium separates arsenic from arseniated hydrogene; and hydrogene, which is supposed by them to exist in both compounds, can have no affinity for hydrogene, nor can hydrogene in one form, be supposed capable of separating arsenic from hydrogene in another form.

Could not the experiment of M. M. GAY LUSSAC and THENARD be explained, except on the supposition of the hydrogene being derived from the potassium, it would be a distinct fact in favour of the revival of the theory of phlogiston. It would not prove, however, that potassium is composed of hydrogene and potash, but that it is composed of hydrogene and an unknown basis; and that potash is this basis united to water.

Other hypotheses might be formed upon the new electrochemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained. Certain electrical states always coincide with certain chemical states of bodies. Thus acids are uniformly negative, alkalies positive, and inflammable substances highly positive; and as I have found, acid matters when positively electrified, and alkaline matters when negatively electrified, seem to loose all their peculiar properties and powers of combination. In these instances the chemical qualities are shewn to depend upon the electrical powers; and it is not impossible that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms.\*

\* Phil. Trans. 1807, Part I. p. 23. The amalgam obtained from ammonia offers difficulties to both the phlogistic and antiphlogistic hypotheses. If we assume the phlogistic hypothesis, then we must assume that nitrogene, by combining with one-fourth of its weight of hydrogene can form an alkali, and by combining with one-twelfth more, can become metallic. If we reason on the antiphlogistic hypothesis, we must assert, that though nitrogene has a weaker affinity for oxygene than hydrogene, yet a compound of hydrogene and nitrogene is capable of decomposing water.

The first assumption is however by far the most contradictory to the order of common chemical facts; the last, though it cannot be wholly removed, is yet lessened by analogies. Thus alloys in general, and inflammable compounds, are more oxidable than the simple substances that compose them. Sulphuret of iron at common temperatures decomposes water with facility, whereas sulphur under the same circumstances, has no action on water, and iron a very small one. The compound of phosphorus and hydrogene, is more inflammable than either of its constituents.

Should a new theory of the dependence of the chemical forms of matter upon electrical powers be established, the facts belonging to ammonium would admit of a more easy solution. Ammonium might be supposed to be a simple body, which by combining with different quantities of water, and in different states of electricity, formed nitrogene, ammonia, atmospherical air, nitrous oxide, nitrous gas, and nitric acid.

Water, on this idea, must be supposed a constituent part of all the different gasses; but its electricities in oxygene and hydrogene would probably be the very reverse of what they have been supposed by M. RITTER, and some ingenious English enquirers.

I venture to hint at these notions! but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtle powers of matter are but just beginning to be considered; and all general views concerning them, must as yet rest upon feeble and imperfect foundations.

Whatever be the fate of the speculative part of the enquiry, the facts however will, I hope, admit of many applications, and explain some phænomena in nature.

The metals of the earths cannot exist at the surface of the globe; but it is very possible that they may form a part of the interior; and such an assumption would offer a theory for the phænomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat,\* and would probably lead to a general hypothesis in geology.

The luminous appearance of those meteors connected with the fall of stones, is one of the extraordinary circumstances of these wonderful phænomena. This effect may be accounted

Water positively electrified would be hydrogen, water negatively electrified, oxygen; and as in the physical experiments of temperature, ice, added to certain quantities of steam by an equilibrium of heat produces water, so in the chemical experiment of the generation of water the positive and negative electricity of oxygen and hydrogen in certain proportions would annihilate each other, and water alone be the result. At all events ammonium, whether simple or compound, must be considered as owing its attraction for oxygen to its highly positive electrical state, which is shewn by its powerful determination to the negative surface in the VOLTAIC circuit.

\* Let it be assumed that the metals of the earths and alkalies, in alloy with common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water, must produce the effect of subterranean fire, and a product of earthy and stony matter analogous to lavas.

for, by supposing that the substances which fall, come into our atmosphere in a metallic state; and that the earths they principally consist of are a result of combustion; but this idea has not the slightest connexion with their origin or causes.